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Continuing Commentary

***On* Zenon W. Pylyshn (1978) Computational models and empirical constraints. BBS 1: 93–127.**

Abstract of the original article: It is argued that the traditional distinction between artificial intelligence and cognitive simulation amounts to little more than a difference in style of research – a different ordering in goal priorities and different methodological allegiances. Both enterprises are constrained by empirical considerations and both are directed at understanding classes of tasks that are defined by essentially psychological criteria. Because of the different ordering of priorities, however, they occasionally take somewhat different stands on such issues as the power/generality trade-off and on the relevance of the sort of data collected in experimental psychology laboratories.

Computational systems are more than a tool for checking the consistency and completeness of theoretical ideas. They are ways of empirically exploring the adequacy of methods and of discovering task demands. For psychologists, computational systems should be viewed as functional models quite independent of (and likely not reducible to) neurophysiological systems, and cast at a level of abstraction appropriate for capturing cognitive generalizations. As model objects, however, they do present a serious problem of interpretation and communication since the task of extracting the relevant theoretical principles from a large complex program may be formidable.

Methodologies for validating computer programs as cognitive models are briefly described. These may be classified as intermediate state, relative complexity, and component analysis methods. Compared with the constraints imposed by criteria such as sufficiency, breadth, and extendability, these experimentally based methods are relatively weak and may be most useful after some top-down progress is made in the understanding of methods sufficient for relevant tasks – such as may be forthcoming from artificial intelligence research.

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Models in cognitive psychology: contrast and constraint. There are two issues to which Pylyshyn (1978a) has drawn attention. Although related, they are sufficiently distinct to allow us to profit from a separate look. The first arises from the more or less friendly competition between artificial intelligence research (AI) and the methodology of cognitive simulation (CS). The second results from the challenge that CP poses for traditional behaviorist psychology (BP). I shall consider the second issue first.

CP vs BP. Recall that it was no less an empiricist than David Hume who observed (1961 [1794], p. 322): "All the objects of human reason or inquiry may naturally be divided into two kinds, to wit, *relations of ideas,* and *matters of fact.*" The former includes "geometry, algebra, and arithmetic," while the latter "are not ascertained in the same manner; nor is our evidence of their truth, however great, of a like nature." Clearly, Hume conceded the legitimacy of the logico-mathematical sciences. Indeed, as Kant tried to show (1956 [1781]), and as we know on pragmatic grounds, the fruitfulness of empirical studies varies directly with the extent to which they are organized by logic and mathematics. Subjects like physics yield much by such organization (termed "deductive-nomological" by Haugeland 1978). But in psychology, mathematical organization has been more modest. Agreement is fairly widespread that the subject matter of psychology differs significantly in *type* from that of physics, and is, in the main, recalcitrant to traditional mathematical treatment. Reductionists, encouraged by early BP success, ignored this. Indeed, they persist in spite of counsel as sage as that of Socrates who noted, "these muscles and bones of mine would have gone off long ago to Megara or Boetia – by the Dog, they would, if they had been moved only by their own idea. . . . It may be said, indeed, that without bones and muscles and the other parts of the body, I cannot execute my purposes. But to say that I do as I do because of them, and that this is the way the mind acts, and not from the choice of the best, is a very careless and idle mode of speaking." (1937, p. 483). Now, as Pylyshyn affirms, CP has mobilized a challenge to BP on precisely this consideration — a challenge backed by two new and promising tools, AI and CS. These constitute a double-barreled approach that (1) adequately countenances the differences in subject matter, and (2) remains sufficiently constrained empirically to satisfy scientific standards.

Pylyshyn argues especially for the second of these points; and though I agree with most of his observations about constraints, that does not seem to be the crucial matter to be argued, for we cannot avoid Hume's assessment: there *is* scientifically valuable analytic knowledge! The epithet "mere tautology" obscures the importance of analytic systems; moreover, if analytic tools are in the right sense appropriate, the necessary empirical links will be found as a matter of course. Thus, what is central in evaluating CP's claim to recognition is not whether AI and CS can be empirically constrained, but rather whether they effectively countenance the uniqueness of psychology's subject matter. And on this score, the situation is quite intriguing. Both use, for the most part, logic – more exactly, the logic of process and control, as opposed to the logic of demonstration. Pylyshyn will hardly disagree here, for he has himself underscored these features both in his target article and in his Response (Pylyshyn 1978b; note especially his comments in sections 1 and 9 relative to Domotor's [1978] observations). And, of course, it is precisely this sort of logic, with its flow-diagram notation, that seems so very well suited to explication and representation of cognitive function.

AI vs CS. Turning to the relation of AI to CS: Pylyshyn argues that the two are essentially similar, differing only in style or in the priority given to various research goals. This seems correct in spirit, but wrong in letter. Correct, inasmuch as the two need not war with one another; but incorrect, inasmuch as they are not without "systematic differences." Systematically, AI and CS are actually complementary, or can be so conceived. To use Humean terms, AI focuses on relations of ideas; it seeks to build "intelligences" of various orders (humans being merely a case of parochial interest) primarily to see to what extent intelligence in general may be a function of ideas integral to information processing, for example, control, feedback, and branching. CS, by contrast, is oriented to modeling certain matters of fact central to human intelligent behavior.

It follows on this view that AI and CS are not equally constrained empirically. While both are adequately so, or can be, they are not constrained in the same way, nor to the same extent. An AI program is constrained in two ways: basically, (1) it must provide for machine-executable structures that accomplish possible intelligent behavior; and (2) to avoid the "merely clever" criticism, it must do so with generality and power comparable to that of some observable intelligent species (preferably humans). CS, on the other hand, is constrained by requiring that (1) its machines, with their programs, approximate as closely as possible specifically human intelligent behavior; and (2) do so in a way as similar as

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possible to the way it is actually done in the human. Thus, CS can and will compete with other approaches to psychological theorizing, while AI will do so only incidentally, being mainly a *systematic* and *dynamic* approach to the epistemological structures necessary or sufficient for carrying out specified intelligent activity, and perhaps, to a lesser extent, an explication of what we mean by "intelligent" activity in the first place.

EDITORIAL NOTE

The author has already replied to this commentary in his Response (Pylyshyn 1978b).

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A broader view of psychology and of computation. Pylyshyn's paper "Computational models and empirical constraints" strikes me as a fascinating and deeply reasoned thesis, especially insofar as it deals with the types of descriptions required to give an account (for example) of an intended behaviour and the neural or mechanical implementation of this behaviour. He is particularly right in pointing out that many of the criticisms levelled at "artificial intelligence" modelling are simply misbegotten, and he has a singularly dextrous finger for pointing with. Another valuable aspect of this paper is the profound, but concise (because discriminating), historical overview.

Pylyshyn is right, too, in noting that the purveyors of A.I. and of psychology have more in common in the field of method than they commonly imagine, and that the debate between "top level sufficiency" and "low level necessity" will continue to yield more polemic than sense *until* there is a suitable unifying theory of conceptual operations and the concrete behaviours they often engender. It is especially fortunate that the author sees no *essential* difference, from the proper perspective, between biological and other hardware.

I could leave it at that (except that commentators are supposed to be constructively contentious) but would be loath to do so because, reading between the lines, perhaps, one finds that the author is probably saying more than he thinks; more, at any rate, than the paper says. The following issues are raised in that spirit, not as criticisms of what is written but as proposed extensions of the argument.

The objection (and I must confess it is a matter of taste in scientific philosophy, and the scope of science) is that Pylyshyn's paper fails to take an adequately broad view of either psychology or computation, even though it is evident that the author has the yen to do so. For example, psychology is more or less equated with cognitive and behavioural science, and computation is more or less equated with a variety of A.I. programs that focus on the serial execution of processes operating upon various data structures (equally, *parallel* processes, if fully synchronised since, under these constraints, parallelism is a technical convenience, not a logical departure from the traditional norms).

So far, so good. But *can* the field *be* restricted in this manner without eliminating the most fruitful interplay of the complementary disciplines?

At *this* level of discourse, it is quite crucial to be sure of the disciplines involved and the boundaries, if any, between them. As a rule, for example, it is acceptable and general practice to talk of "behaviour" or "cognition" as though they were psychology. Clearly, they are not, although it is quite true that behaviourism is *a* psychology (a school of thought about psychology) and, by the same token, cognitive science is *a* psychology. At *this* level of discourse, it is necessary to say what is clear, namely, that behaviour, cognition, and the like are *not* psychology.

I doubt, for example, if anyone has encountered a pure and pristine cognition; a rational thought, devoid of conation, affect, and the like. I doubt if such a thing exists and am suspicious of a science of mind that does not embrace (rather than exclude) such ubiquitous phenomena as consciousness.

Similarly, *process* in general is *not* Turing representable. A nonclassical (action valued) and many sorted model theory is required, at the least. For example, I am suspicious of a science of computation that does not adumbrate biochemical computation (as in a biological cell) or neurophysiological "abcission" (the term is due to Grey Walter; it comes from an early paper on the contingent negative variation, CNV, and related indices of neural activity, written in the 1950s).

These comments are relevant, quite clearly, to the forms of empirical constraint that Pylyshyn has in mind, especially their impact at the real world interface with programmed systems; also, to the discussion of "specificially psychological constraints" upon computer models (the convenient categories of "intermediate state evidence," of "relative complexity evidence," of "component analysis evidence") and bear more obliquely upon nearly all aspects of the discussion (for example, "generality," or "level," of analysis). They amount to an appeal for discussion, in some paper to come, of a much more fundamental approach to these matters than is usually countenanced; for we live in an age predisposed to overemphasise the relatively pedestrian examination of a rich and wonderful universe (albeit many sorted and dynamic), which is filtered through the blinkering spectacles of technical expediency.

Pylyshyn has written a perspicuous paper. My suggestion that he follow it by a deeper one in the future is due, to some extent, to dissatisfaction with both the mainstream artificial intelligentsia and the narrow experimentalists, which (reading between the lines, as well as along them) the author seems to share.

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Modulators, Mediators, and Specifiers in Brain Function

Interactions of Neuropeptides, Cyclic Nucleotides, and Phosphoproteins in Mechanisms Underlying Neuronal Activity, Behavior, and Neuropsychiatric Disorders

edited by **Yigal H. Ehrlich, Jan Volavka, Leonard G. Davis,** and **Eric G. Brunngraber**
The Missouri Institute of Psychiatry

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